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ABSTRACT

The author examines, in the light of funding problems for Federal agencies supporting educational research, the difficulties inherent in the research process as it now exists, and methods for attacking these difficulties. The thought is presented that only by working on these general problems, will the more specific problem of funding be effectively solved. Comments are presented on one aspect of educational research: that it is perceived as as having little impact upon practice. Explanations of this perception center on (1) the role of dissemination, and the relatively little funding devoted to it; (2) the role of research as a legitimizer of practice, rather than as an initiator of change; (3) the gap between researchers and research users, in which researchers are the "professionals" and users a less knowledgeable "public"; (4) the nature of scientific models, in opposition to the humanistic concepts employed by users; (5) the problem of "proof" and "truth" in educational research--when is something "proven true"? Suggested remedies include (1) increased attention to dissemination; (2) increased attention to the demonstration of theoretical sources of practice; (3) increased emphasis on research rationale, the development of theory, and the integration of research into new wholes; and (4) attention to bridging the gap between researcher and user through increased awareness of the gap and its effects, through the use of classrooms for research settings, through an emphasis on success in practice as the final proof of research validity,. and through modification of research development models to include more teacher involvement. (MJB)

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An Analysis of the Perceived Ineffectiveness of Educational Research and Some Recommendations. 73
David R. Krathwohl

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Mr. Average Citizen is no longer willing to write blank checks in support of research. Despite this, in the funding crunch of NIE, we found a basic confidence in research that helped prevent that organization from going under.

This fundamental confidence is justified by its role in suggesting new possibilities for education, in its elimination of ineffective practice, and in its providing evidence for some of the really important decisions regarding educational policy. For example, its role in the Brown vs Ferguson decision was fundamental to the whole desegregation decision. In reading the following article it is important to remember the very positive role that research plays.

Nonetheless, as the initial statement notes, there are problems. The article concentrates on these problems, in part as a possibly healthy antidote to efforts to oversell research, but mainly on the assumption that the first step to the solution of a problem is its identification.

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AN ANALYSIS OF THE PERCEIVED INEFFECTIVENESS OF EDUCATIONAL RESEARCH AND SOME RECOMMENDATIONS[1]

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ABSTRACT

While not the usual type of presidential address, this paper follows a conviction that began with a similar presidential address to the American Educational Research Association, that it is more important to use this occasion to consider critical problems confronting the profession than to report personal research. Because of the leadership role that educational psychologists have traditionally played, the consideration of these problems, which affect the entire educational field, seemed particularly appropriate.

Support of Educational Research: A Crisis

A few years ago, educational research, like other research areas, was growing in funding and we were looking forward to a continued development of a long underdeveloped aspect of professional education. That situation no longer exists. Resources available to the National Center for Educational Research and Development and to its successor, the National Institute of Education, plateaued at about $120 million several years ago and, in addition to the effects of inflation, were reduced to $75 million this past year. More recently the Senate approved zero dollar funding for NIE! Representative Edith Green received a standing ovation from her colleagues in the House following an impassioned speech which was climaxed by a motion to cut the budget of NIE!

To make matters worse, there has been no widespread sympathy to change this state of affairs. To be sure, farsighted supporters of educational research—like Representatives Brademas and Quie, and Senators Stevens, Eagleton and Javits—have tried to turn the tide; but it has run strongly against them. Indeed, the President of the American Federation of Teachers sent a letter of *congratulations* to the Chairman of the Committee on Appropriations when they cut NIE's budget last year.

While few research programs are receiving lavish support these days, it seems safe to say that educational research has suffered more severely than most. One may take the point of view that, as one of the last of the areas to be funded, it is less well established than other areas and therefore more vulnerable. Certainly it is true that it has not devel-

1 A revised version of the Presidential Address to the Division of Educational Psychology at the Annual Convention of the American Psychological Association, New Orleans, Louisiana, September 1, 1974. The author's address is School of Education, Syracuse University, Syracuse, New York 13210.

Performance-Based Teacher Education Project
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oped the base of popular support that other programs have, despite the importance of education to our economy and to our society. This suggests that we would do well to take the current actions and attitudes toward educational R and D as symptoms of more basic underlying problems. Certainly we should at least try to better understand these underlying problems and, to the extent possible, find ways to remedy them. It has been said that a problem well stated is half solved. This paper is an attempt to state our problems and to then look at possible suggestions for their amelioration.

Have We Oversold Educational Research?

One of our critical problems is that more seems to be expected of educational R and D than it has delivered, possibly more than it can deliver. There have been no widely ballyhooed break-throughs, no startling successes, no innovation that has swept the nation. Senator Claiborne Pell, Chairman of the Senate Appropriations Committee, which controls educational research appropriations, commented recently to a convention of educational laboratory and center directors on the plight of educational research. He noted that they may have to explain why, after a generation of R and D children may actually be learning less though staying in school longer. Whether accurate or not, here is a clearly stated perception of lack of accomplishment by a key figure. Further, he went on to indicate that what was needed was to do a good lobbying job and come up *with a couple of startling examples of success* (underlining mine) (Pell, 1974).

Nothing on the horizon that I am aware of suggests that we have "startling successes" on the drawing board. Incremental progress, yes! Startling revolutions? No. Medicine, physics, most of the other areas using the scientific model, do turn up occasional quite unexpected results. Ours nearly always have an element of the expected or "common sense would have told you that" about them. (The fact that "common sense" might have suggested equally plausible alternative solutions is rarely pointed out.) This suggests that expectations of educational research are going to somehow have to be adjusted to the realities, or we will continue to have difficulty getting funded.

Why is Research Perceived as Having Little Impact?

The Role of Dissemination:

It is hard to tell whether the lack of dramatic breakthroughs in educational research is an inherent characteristic of that research or merely characterizes what we have done so far. While we can agree that the apparent impact of research on the field of education has not yet been dramatically apparent, one must also look at the size of the research and development effort in comparison to the size of the field that it is intended to affect. Less than 1% of the funds devoted to education are spent in R and D, including the major dissemination efforts related to that R and D. It seems clear that far from adequate funds have been devoted to disseminating those things which have been developed.

It is difficult even for those persons relatively acquainted with the Federal educational R and D effort let alone Congress and the general public, to cite products that have been developed. This is not because the products are poor, but because of the very minimal effort of dissemination. Dissemination is so extremely expensive compared with the research and development process that when the two are in the same budget,

one or the other is likely to get short changed. Considering the size of the total budget, it had to be dissemination.

One can safely claim that a more massive R and D effort, even without "startling breakthroughs" would have had greater impact if for no other reason than that there might have been more adequate funds for dissemination! That in itself would give a different impression of educational R and D than the present one.

Research as a Legitimizer of Practice:

In addition to the small size of the dissemination effort, the role that research has played in relation to practice has contributed to the perception that it has little impact. It was noted earlier that one of our problems is to change our expectations of research. One of those expectations is that a particularly important piece of research will initiate a whole change in the school practice picture–a brilliant piece of research would initiate a process of development which would be followed by massive dissemination and adoption. But it is probably safe to say that the most sweeping school changes don't follow this pattern.

Instead of being a leader of change, research in education more often serves as a legitimizer of change. Take for example, competency based teacher education. While no one knows for sure from whence competency based teacher education (CBTE) came, the roots of it, in terms of behavioral objectives and measurement of those objectives, have been around for a long time. The thirteen model elementary teacher education programs that were developed at USOE instigation and funding, gave these characteristics new impetus for changing teacher education. Nearly all of these models independently developed competency based programs with stated objectives and follow-on measures of these objectives.

It seems likely that these models would have passed on to obscurity as utterly unachievable ideals except that they were preceded by the research of Ryans, Flanders, Medley, Mitzel, Soar, and others in the field of classroom observation scales. They had begun to link the action of the teacher in the classroom to the effectiveness of the student's learning. While these findings were embryonic, the fact that they existed at all made it seem possible that in time a competency based curriculum might indeed be a reality. Further, this hope provided a combination which served the needs of State departments of education and others who, discontent with the effectiveness of public education, viewed teacher education as a way of improving that situation. The current mandating of competency based teacher education, in eleven states at present, carries the movement far beyond its roots in research and completely loses sight of the realities of bringing it to fruition without much more extensive research. Yet, the movement is in full swing with colleges and schools of education throughout the country converting to competency based programs.

In a similar manner, Bruner's summation of the implications of research on cognition (Bruner, 1960) provided the framework within which physical and biological scientists, interested by NSF in reforming the schools' science programs, could legitimately exert their expertise. Extensive National Science Foundation funding provided the means for their doing so.

Still another example is Skinner's work with teaching machines and programmed

learning (Skinner, 1958). Skinner pointed the way to the better and more effective use of teachers at a time when there was a marked teacher shortage. It suggested ways in which the teacher's energies could be used more effectively and fewer teachers needed.

In each of these instances (and one could cite others), the work of one or more educational psychologists provided research evidence that supplied a rationale for movement in a direction in which there was momentum. Each movement succeeded in mobilizing tremendous energies and has had a substantial impact on educational practice.

The point is that in none of these instances were the practices that used the research as justification confined to the level of certainty of the research results. (For that matter, some, such as Skinner's, over-generalized animal learning to humans.) In each instance, research became a legitimizer of practice. It preceded the change in practice, it gave the change impetus, it legitimized the change in the eyes of others who might question it, and it facilitated its acceptance by those resistant to the change.

Practices may have a basis in research, but often much innovation in education grows out of practices that seemed successful or that on some logical base ought to be. If the practice had a base in research that basis has usually been lost in the intervening steps to the development of the practice. Evaluation research is a legitimizer of whatever practice is shown to be the most successful.

These examples suggest that one of the reasons for the lack of centrality of research in educational change is that it frequently serves, not so much as the initiator of change, but rather as a legitimizer of it, a much less obvious and glamorous role. Practice proceeds as rapidly as the inventive mind of the practitioner can push it. Viewing teaching as an art which is to be practiced to the best of one's capacity, the teacher and administrator cannot wait for research to explain where and/or why something must be done, but they must do it. Research comes along later and shows them whether they were right in believing they improved practice, and frequently suggests why certain practices have worked. This lends ever increasing solidity to our educational practices. It serves to delineate a practice that works, "that is just plain common sense" from another practice which is equally apparently justified on a common sense basis. It provides us with products which have been demonstrated to achieve certain stated goals. All of these are substantial accomplishments. But they can hardly be characterized as "startling breakthroughs." The fact that they characterize the bulk of educational research and development means that we must re-characterize the expectations of educational research and development when we talk about it to others who would otherwise expect of it a much more dramatic role.[2]

The Gap Between Researchers and Research Users

Still another reason for the seeming lack of impact of educational research is the gap between the researcher and those who use the research and development products. This gap at least in part results from the models that we have chosen for our research and development processes.

Joseph Haberer, in a fascinating article on "Politicalization in Science," (Haberer, 1972) notes that: "A special relationship exists between a profession and society. A profession is a socially rooted and supported vocational enterprise of full-time practi-

2 I am indebted especially to Dr. David Clark of Indiana University, who shared with me the discovery of this insight on the role of research in the course of a committee meeting discussion.

tioners who earn their living by providing a vital social service through the utilization of expert and esoteric skills. They are granted substantial autonomy in conducting professional affairs . . . based on the recognition that a profession operates in a realm of expertise which those outside of it can make no claim to and which they, at best, understand only very generally. For non-professionals, therefore, this requires a considerable degree of dependency and trust–if the professional is (to be free to) to perform his work satisfactorily. A reciprocal obligation, however, is placed upon a profession; namely, to fulfill those responsibilities which it has either explicitly or implicitly assumed" (p. 720).

This describes quite well the roles which have been adopted by researchers and research users, where researchers are the "professionals" and the users, though also professionals here assume the role of the "public." Users, following the role assigned the public by Haberer, typically do not pretend to have the statistical, experimental design and measurement knowledge of the researcher. Researchers, in turn, have assumed the role of scientists and have believed that the proper practice of science was all that was required of them. By patterning their work after the hard sciences, they would participate in the successful role model of the age and share its accolades. Truths about education would be proven, and educational research would receive the same kind of positive and extensive support as have other sciences.

But those accolades have failed to materialize, and the extent of faith in researchers as professionals fulfilling their trust has often been modest at best. The current situation better resembles that described by Haberer when society's expectations of the professionals have not been fulfilled. Further, this has occurred despite conscientious efforts by researchers to perform their roles to the best of their ability!

Characteristics of Scientific Model Contributing to the Problem:

This latter comment suggests that part of the problem may lie in the nature of those roles which creates a gap between the researchers and the research users. In imitating the hard sciences, we have adopted a method with two important characteristics: first, an adoption of the scientific method, giving it a first priority over other values; second, the use of a linear process of development in which new knowledge is validated, engineered or developed into a practical format for use and dissemination to users for adoption.

Emphasis has been placed on the scientific method results, according to Haberer, in training which stresses ". . . to the exclusion of everything else, technique and method" (Haberer, 1972, p. 721). The result is instrumentalism and detachment; quite a contrast with the view of professions as special callings, vocations that entail service to human needs. This impacts, of course, on the image that researchers project as they engage in their work in settings shared with others in the educational profession who, by the nature of their responsibilities, are more closely related to the human services aspects of the field. It tends to divide the profession into thinkers and doers, a concern long expressed by practitioners about staffs of schools and colleges of education. It is no doubt further emphasized by the personalities of those staff members attracted by the scientific model into educational research training programs. They contrast particularly with those attracted to education as humanistically oriented personalities seeking to serve human needs.

The gap is also a function of the development process, which involves scholars and

scientists in the discovery and validation of findings at the beginning of the process and at the other end involves teachers and users in dissemination and adoption. With the researcher primarily involved at the beginning and the practitioner at the end of the developmental process, the role of the researcher is removed from and in large measure concealed from the consumer. As a result, the kinds of research considerations which go into the preparation of a curriculum or a self-instructional classroom, or a psychological or educational test are rarely set forth even for selling purposes in a way that indicates the extent to which they were actually employed. Thus, when the consumer is asked about the impact of research on development products, he can honestly answer, "little or none" since he has no way of being aware of the true situation.

Another characteristic of the linear development model that contributes to the researcher-user gap is that it takes a long period of time from beginning to end if each step waits for the previous one to be completed. Discovery is followed by confirmatory studies. These in turn are followed by an intensive and often prolonged period during which the new knowledge is engineered for use in practice. Generally this involves a few teachers and practitioners in field trials, but there is no general availability of whatever is being worked on until it is perfected to a satisfactory level. Once that level is reached, there then follows a period of dissemination and adoption which, as the NSF learned with their curriculums, is a much longer and more complicated process than surface appearances would indicate.

There is continual pressure to shorten the initial stages of this process which is resisted by the true empiricist. He is troubled by missing and/or too markedly abbreviating the confirmatory steps, for he fears users will base their actions on research which later will be invalidated. Indeed, even if no harm were done to the consumer by use of a product later found to be invalid, such an occurance gives research a bad image. Potentially it could sour the public on support of research. For similar reasons, he may be equally dismayed when work in incomplete stages of development is put to use and fails. It then reflects badly on the developers and the processes they used.

But the process is a very long one for practitioners faced with the daily press of problems that research is supposed to help alleviate. It operates in a time frame which is a world apart from the one in which he must operate.[3] And the fact that the process sometimes is extended because the developer will not release a product until it is engineered into a "teacher proof" package makes the user doubly resentful, first because of the period the product is withheld from use and, second, because of his implied incompetence which results in the need to "teacher proof" products. Clearly these are formidable problems, in some measure at least an integral part of the models we have adopted for use.

Thus we can see that one of the causes of the perception of lack of impact of educational research is the gap between researcher and research user which keeps the latter ignorant of what the researcher is doing and which in some instances causes the user to distrust the researcher as really having his interests at heart. The gap is, in part at least, a result of the inherent nature of the models of research and development that we have

3 Congress too is pressured for problem resolution and the House of Representatives faces a "vote of confidence" every two years. Thus, it also is pressured to operate in a short time frame, a fact of considerable importance, since they authorize the bulk of research and development resources.

chosen to use. In some instances these models may be used in ways that accentuate the gap, or are at least used by persons who by nature may be more interested in the research than that such a gap exists.

Will Educational Research, Like Other Scientific Research, Discover New Truths?

We have led people to believe that research would prove truths about education, just as such truths are proven in the physical and biological sciences. But it appears to be much easier to prove things in the physical sciences than in education. To most of us, the physical science subject matter is abstruse, complex, and mostly outside everyday life experience. By contrast, education may be every bit as complex, but we do not yet have the conceptual schemes to adequately describe this complexity. Further, the subject matter is much more likely to be a part of each individual's past history as well as the stuff of his current experience. Thus individuals feel much more qualified to challenge the so-called expert and to accept, or not accept his findings. They subject them to an inner test of some kind to check their veracity.

Recently I ran into an interesting example of this. One of our colleagues recently advanced the Pygmalion effect as a potentially potent strategy for use with inner-city children. I was surprised by this, since the Rosenthal and Jacobson (1968) data only barely support the effect in the early grades, and do not at all support it in the later grades. The author did indeed indicate that their study is controversial and the data are weak. But then he concludes by noting that we all know of instances in which the effect, however, has worked!! This merely reinforced for me the judgmental nature of "when is a principle or finding proven," or for that matter "when is it contraindicated." Here we had one of our more interesting senior investigators using personal experience as a basis for his judgment about a research finding.

Let me cite another example, one in which a whole field appears to have ignored research evidence: the work of Travers with respect to the effect of media on learning (Travers, 1968). He notes a large number of studies which indicate that learning is facilitated by the use of two channels of information reinforcing each other–usually sight and sound, as in the case of instructional films. Travers himself, however, is convinced that we are capable of processing only one channel at a time, and that we actually lose by presenting material simultaneously through both channels. He proceeds to demonstrate with a series of very convincing studies that this prime tenet of the audio-visual field appears to rest on a fallacious assumption. Has Travers overturned a basic assumption of the field? Think about it for a moment. When is a finding proven? or disproven? after one study? two? three?

Since this is a prime tenet of the audio-visual field, why have these studies not made more impact? Is it that the methodology is not acceptable? Certainly we know that one man's tight design may leave variables uncontrolled that another may find very distressing. But Travers' study is unusually tight, by any standards. More likely it is because this finding runs counter to the general context of belief in the field, and therefore people are going to require more evidence before they are convinced.

Factors in the Acceptance of Research as Proof of Truth:

It is hard to say exactly what causes a finding to be considered proven in education.

It is reasonably safe to say that repeated studies demonstrating the same findings, using different experimental designs, different samples, different subject matters, different subjects, etc., are sufficiently convincing. But it is very rare that a finding is given such thorough investigation. Few findings seem sufficiently important to warrant it.

It is clear that judgmental factors play a heavy role. Though in some instances these factors may be influenced by the context of beliefs about an area, as we may surmise might well be the case involving Travers' research, more often the judgment is based on how close the evidence is to one's own or related experience, as in the "Pygmalion effect" research.

This problem is not unique to education. For example, researchers knew that hybrid seed corn was superior to that in common use. But they could not convince farmers to use it until there were extensive demonstration plots throughout the country where a large number of farmers could have personal experience with its effects. A comparable situation in education is the spread of instructional television. Researchers quickly proved it could be used for instruction, but this had then to be redemonstrated in each subject matter area for the results to be accepted by those working in that area.

In education, however, we have an additional problem over agriculture or medicine. Our treatments are either so weak that they do not show strongly in most situations or they are so interactive with an individual's capacity to learn that the student's accommodation to the method makes it difficult to determine the actual contribution of treatment. As a doctor friend of mine noted, one does not need statistics to sense the impact of penicillin. Most of our treatments show up only under statistical analysis (some don't even show then!). This deprives the observer of first-hand sensed impressions which more strongly than other evidence influence judgments of effectiveness.

The matter of acceptability of proof is closely related to the matter of dissemination. It was noted earlier that the dissemination problem was more complicated than it appeared to be. The whole concept of the validation and dissemination of knowledge is a very complex problem to which we have given too little attention, particularly since the difficulties are increased in a discipline as "transparent" to the lay public as ours. Educational researchers in general are not, and cannot expect to be, accepted by others as knowledge producers in the same sense that other scientists are. Neither can the research community assume that their responsibility ends when they complete a study. But just what their role can best be is still to be determined.

This whole area is one which must receive priority attention if research and development is to have a chance of fulfilling its promise for the betterment of education.

Recommendations Which May Improve the Perceptions of Educational Research

Let us stop the analysis here. This is perhaps enough to suggest some of the bases for our current problems. If these bases have been in any way accurately identified, then we can begin to consider possible courses of action for remediation—some of which have been suggested in the course of the analysis.

Specifically, what actions should we take? Here are a number of suggestions, some of which hopefully may catch your interest.

1. An Increased Dissemination Program—We must begin paying a great deal more attention to dissemination of the results of research and development. This statement

has many implications. For one thing, it means that we must seek a much larger commitment of resources so that a substantial dissemination effort can be mounted. Such an effort is impossible with the kinds of resources that currently are being allocated. For another, it means that we must seek to build a much better dissemination apparatus. Many suggestions have been made—persons with special responsibility for interpreting R and D in each district or perhaps even in each school building, more demonstration projects, persons comparable to the county agent in agriculture, to name but a few. The very plethora of suggestions suggest that one of the top priorities must be the mounting of a substantial program of research in the area of dissemination itself, so that we can better learn how to do it.

2. Improved Understanding of the Links Between Research and Practice—We must do a better job of knowing the links of educational research to practice, both historically and contemporaniously, and do more to make these links known. Our teacher preparation courses do not trace ideas and conceptions to the research from which they come. Neither do we do much with the history of education, which would note such roles as that of the early as well as recent educational psychologists. Responsible for some of the earliest work on learning, it is fair to say that the field has had an influence on the curriculum of the schools and colleges almost from the birth of the field of psychology through the present day. This year's Thorndike Award winner, Dr. Robert Gagné, for example, guided the curriculum, "Science: A Process Approach" from its inception. We have not made these contributions apparent.

This problem is likely to be especially great when one considers what is happening in performance or competency based education. In some instances it is being limited to that knowledge required to learn proper teacher performance. Not only may the theoretical bases of that performance be omitted, but it seems almost certain that tracing those bases back to their research underpinnings may well be slighted. Certainly if this occurs it would intensify the very problem we are trying to overcome. As educational psychologists involved in teacher training, we should bear this in mind, both as we prepare CBTE modules for our own courses and as we work with our colleagues in the overall design of our CBTE curriculum.

Jack Getzels in a recent article provides an excellent example of what can be done to help practitioners understand the links of research to practice:

> "Almost within sight of my office are four school buildings. In one, dating from the turn of the century, the spaces called classrooms are rectangular in shape, the pupils' chairs are firmly bolted to the floor in straight rows, and the teacher's desk is front and center. In the second building, dating from the 1930s, the classrooms are square, the pupils' chairs are movable into various patterns around the room, and the teacher's desk is out of the way in a corner. In the third building, dating from the 1950s, the classrooms are also square but the pupils' movable desks are now trapezoidal in shape so that when they are placed next to each other they make a circle, and the teacher's desk has vanished! In the fourth building, there is a classroom, constructed a year or so ago, that is four times the size of the ordinary classroom. It has no teacher's or pupils' desks at all but is filled instead with odds and ends, from fish bowls and birds' nests to drawing boards and Cuisinaire rods. If one were not told it was a classroom, this space might be mistaken for an overgrown playroom or a warehouse full of children's paraphernalia." (Getzels, 1974, p. 527-528).

In the remainder of the article he traces each of these classroom forms to a conception of the learner—conceptions based on educational psychology research: the rectangular room to the early empty learner-connectionist conceptions; the square room to the active learner conception that involved Gestalt psychology and research on affective learning; the circular classroom-the social learner conception to social psychology research and group dynamics; the open classroom-stimulus seeking conception to recent research on the individual as not only a problem solving, stimulus-reducing, organism, but also as a problem finding, stimulus seeking, organism.

Getzels, by taking common practices and tracing them to their research roots, and then publishing this material in a journal which will reach practitioners and administrators, helps to bridge the relationship between research and practice. Developing additional articles that trace the roots of practice, as Getzels has done, would similarly contribute to a solution to this problem. It is only a beginning, but it leads in the right direction.

Concomitant with demonstrating the relation of practice to research, we must extract from that material some understanding of how the research and development process works and convey this as well. This will include some modified understanding of the kinds of contributions that research can make, and note the incremental way in which it undergirds practice. It will note its role as a legitimizer of practice. It will indicate why that role, though a less glamorous one than might be anticipated for research, is one that reasonably follows, both from the professional roles that teachers must practice and from our limited present understanding of the educational process.

Articles which discuss these matters may help *all* of us to better understand these relationships. They may call the attention of some of the decision makers to these problems, so that they may be better informed when they consider educational legislation, appropriations, or research and development program decisions. And certainly we must do more to inform our students of these problems, so that being aware of them, they may in their future work assist us in finding better solutions to them.

3. Increased Emphasis on Rationale and Theory Building as Convincing Contexts for Research Findings—One of the factors which enters into the acceptance of a research finding as a basis for practice is the extent to which it is embedded in a plausible explanation or theory. The examples of Bruner and Skinner mentioned earlier show this. McKeachie has made an excellent analysis of this phenomenon in the case of Skinner (McKeachie, 1974).

Emphasis on research rationale, the development of theory, the integration of research into new wholes, are important goals of educational research. Yet we do little to emphasize work that moves in this direction. For lack of space, our professional journals tend to cut such material from research reports. The *Review of Educational Research* is to be congratulated in its new format for getting articles that serve to bring research together. But precious few articles emphasize the overall conceptualization that encompasses the research or explores new conceptualizations. Further, where is the research journal where one would publish a theoretical piece that does not contain data? Division 15's new format for *The Educational Psychologist* is the only one I know. This is a good start, but further outlets are needed. Clearly, an emphasis on this kind of thought needs to be further reinforced in terms of our current publishing practices.

Suppes suggests still another way of emphasizing theory. His presidential address to AERA in 1974 (Suppes, 1974) dealt with the importance of theory. He suggested that in this day of specialization, there should be persons in educational research with a special involvement in theory development. Noting that "physics has long recognized such a division of labor between theoretical and experimental physics. . ." he argues for the encouragement of a similar division of labor in educational research. Further, the absence of such a division of labor in educational research, he argues, is "a mark of the undeveloped character of current educational research." This suggestion seems worthy of careful consideration.

One may argue that we are not now ready for such specialists in theory development, and that they will emerge when the field has developed to the point where they can be usefully employed; our current theories are too simplistic. Perhaps so, but one may wonder if this may not be a chicken and egg situation. May not the state of the field at least in part be due to the fact that up to the present time so little trained attention has been given to it?

4. Bridging the Gap Between Researcher and User–There are a whole series of suggestions which may help to narrow the gap between researcher and research user, especially the practitioner-teacher:

A. Increased Awareness of the Gap and Its Effects–Perhaps, as in the earlier suggestion that better understanding of the role of research may lead to a better situation, so awareness of the gap between the practitioner and the researcher, and why it exists, may result in more effective efforts to bridge it. Building on articles such as this one in greater depth would contribute to this goal.

Certainly such awareness ought to motivate researchers to more direct efforts in this regard. If nothing else, were it to result in more conscientious feedback to participating schools of the results of research conducted in those schools, this would be a substantial step forward. Too many researchers ignore this simple but basic courtesy! If one were to go beyond that in such feedback sessions, and try to impart some perspective on the whole research process–what it can and cannot do–using the research in question as an example–still more might be achieved.

B. Modification of Research Methodology–We can modify the extent to which we depend so heavily on experimentation as a research method, and return to the classroom as a setting for our work. Brophy notes this in suggesting that educational psychology should be more concerned with teaching than with learning (Brophy, 1974). The work of Phillip Jackson (1968) and Smith and Geoffrey (1968) indicate the richness of data drawn from observation of the classroom. Besides resulting in research which is immediately more understandable to the practitioner, it results in work on problems of considerably greater relevancy to educational practice.

C. From Knowledge Producers to Producers of Findings to be Confirmed in Practice–We can modify our view of ourselves as knowledge producers toward that of producers of findings to be confirmed in practice. This suggests that, rather than presenting our findings as proven, our presentations should make clear the tentative nature of our findings and routinely seek the help of practitioners in further validating or invalidating these findings, suggesting the simplest possible methods for so doing. Such suggestions could be included in our articles in professional journals, as well as

being carried on by the researcher himself. The former is a simple change, and not costly, but a very important one! We routinely make suggestions to other scientists for further research, but have ignored the validating role of the user. Were we to routinely include him, it would be indicative of a change in our attitude about the practitioner and it would get more material more quickly into practice. It would also confirm and make more apparent where the ultimate test of validity lies.

Taking the ultimate validity argument further might change publication from a game of "How many articles have you published?" to "What has been the real significance of that research in practice?" The real pay-off for research would not be so much as at present the publication of the research itself or its presentation at a convention, but its value in practical terms as measured by its effect in the schools. Many things would have to change before this could become a reality, and considering the time line involved for the implications of research to become known, it probably is not practical. Yet, if speculations about the future answers to this kind of question were asked with respect to academic work submitted for consideration with respect to faculty promotion, pay raises, and tenure, certain less productive aspects of the academic game might be minimized.[4]

Still another implication is that reviews of research could be combined with reviews of practice. Recognizing that knowledge comes from practice as well as research, and that the ultimate validation of research is in practice, reviewers of research should be encouraged to search out confirming or disconfirming evidence from practice to set alongside research findings. As research articles include suggestions for practitioner validations, such reviews could then become the collating points for practical evidence. At first they might be largely case studies, later collating and summarizing case studies as that is possible, and in turn using still more sophisticated evidence from practice as it becomes available.

D. Modification of the Developmental Model—In a similar vein, developers may wish to consider modifying their model. We have noted the difficulties involved in the long development time line and in the production of "teacher proof" products. For one thing, developers may wish to research the most appropriate level of development relative to "teacher proofness" in terms of kinds of teachers, schools, and instructional goals. If materials can be put into the field at earlier stages of development, this would both reduce the time and expense of development, as well as give teachers more of the creative and intellectual challenge which many of them want as growing professionals.

They may also wish to try alternative models of development which rely more heavily on teacher involvement, and which are as much or more process as they are product oriented. Such models might involve the simultaneous development of curriculum or other innovation at a variety of teacher centers around the country, selecting the best of the ideas and/or materials devised under these conditions, and the interchange of these ideas and materials among the centers. Then, contrasting the ter-

4 I can anticipate this comment raising significant protest. Such protest would be quite legitimate from those who are concerned, as I am also, with the promotion of basic as well as applied research. But I am concerned here, not with basic investigations for which questions such as the above make no sense, but with that applied research which, by its very conceptualization, leads primarily to a journal article and no where else.

minal costs of such a development process with the costs of the currently used central product development process plus the costs of effective dissemination, may be helpful in determining something about the characteristics of the model we should be perfecting.

E. Other Suggestions—Numerous other suggestions for narrowing the gap between researcher and practitioner could be made: establish panels of practitioners to suggest research priorities and ideas; provide ways for researchers and practitioners to more easily exchange roles, as in Russia where researchers take teacher roles to try out ideas; use practitioner panels to review research and choose items for dissemination.

Perhaps enough has been mentioned to suggest that the gap can be bridged if we consider it important enough to do so. Certainly an essential element in that bridging will be a better understanding of the role of research and its potentialities in relation to practice. But it seems likely that an equally essential element will be finding a way for those practitioners who wish it to find a meaningful role in the R and D process, so that they may get the fulfillment of using their considerable professional skills and share in the feeling of excitement of contributing to the cutting edge of improved educational practice.

The Plight of Educational Research Support vis-a-vis the Linkage of Research and Practice

Let us now return to consider the plight of the support of educational research. With one exception, none of the above suggestions directly attack this problem; rather they seek a better understanding of the role of research and development in educational practice, and the enhancement of that role in such a way that the positive impact on practice is increased. By now my belief must be obvious that only as the problem is attacked from this orientation will we bring about an improvement in the resource picture.

I am very much concerned that the temporary relaxation that may result if budgetary considerations for NIE improve this year (which I think they will!) may lull us into thinking the crisis is over. We might then continue about our business as we have in the past. The current NIE crisis is only one symptom. Without an improvement in the perceived relation of research to practice, our practitioner colleagues will have no more basis for supporting R and D funding in the future than they have in the past. Without such support, funding is less than likely to be stable, let alone increase markedly. This is a problem that will occupy our sustained attention for some considerable period of time.

I wish I could claim that the recommendations set forth in this paper constitute a program for moving forward. Not only would such a claim be pretentious, it would be far from true. My hope is that there are other and perhaps better analyses and recommendations that could be made but these are a start, and hopefully will turn us in the right direction. If they leave you with enough discontent that they stimulate your thinking and acting along similar lines, this paper will have served a purpose. For what it is worth in terms of motivation, such effort is to your own self interest as a researcher. More important, however, it is also for the greater good of education as a whole. On behalf of all of us, I hope you are stimulated to further thinking about the problem, and I wish you much success in your problem solving.

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